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Voter Turnout and the Size of Government

Linuz Aggeborn

Uppsala Center for Fiscal Studies
Department of Economics
Uppsala University
P.O. Box 513
SE-751 20 Uppsala
Sweden
Fax: +46 18 471 14 78

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LINUZ AGGEBORN

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Voter Turnout and the Size of Government *

Linuz Aggeborn[†]

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Abstract

This paper investigates the causal link between voter turnout and policy outcomes related to the size of government. Tax rate and public expenditures are the focal policy outcomes in this study. To capture the causal mechanism, Swedish and Finnish municipal data are used and a constitutional change in Sweden in 1970 is applied as an instrument for voter turnout in local elections. In 1970, Sweden moved from having separate election days for different levels of government, among other things, to a system with a single election day for political elections, thus reducing the cost associated with voting. This constitutional reform increased voter turnout in local elections in Sweden. The overall conclusion of this paper is that higher voter turnout yields higher municipal taxes and larger local public expenditures. Second, there is some evidence that higher turnout decreases the vote share for right-wing parties.

Key-words: Voter Turnout, Size of government, Sweden, Finland, Local public finance, Instrumental variable regression

JEL Classification Codes: D72 D70 H39

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[†]Department of Economics, Uppsala University. Uppsala Center for Fiscal Studies; Box 513, 751 20 Uppsala, Sweden. E-mail: linuz.aggeborn@nek.uu.se

1 Introduction

A number of democratic countries have experienced a fall in voter turnout rates since the end of the Second World War.¹ Understanding the reason for this decline has been a major research question within the social sciences; voter turnout is considered by some to be important in order to sustain the legitimacy of the democratic process (Lijphart, 1997, p. 1-2). Certainly, there exist several normative arguments as to the importance of voter participation, as well as arguments reflecting a more skeptical stand regarding the value of high turnout rate.² Nevertheless, voting is not only an isolated political action linked to the issue of democratic legitimacy but also, at least in theory, the basis for the formation of public policy. The purpose of this paper is to investigate the latter by taking a more positive approach that examines the causal effect of a variation in voter turnout on public policy. This paper is especially focused on policy outcomes related to the size of government, such as tax rates and public expenditures.

There is an ongoing discussion within the fields of political science and political economics over whether certain parties are favored when voter turnout declines.³ The focus in this case is whether a decline in voter turnout results in unequal representation. Regarding the actual effect on policy outcomes, there are two potential mechanisms through which turnout may affect policy. First, a drop in voter turnout may affect the policy position of all parties running in an election as a result of an alteration of the median voter's position. In accordance with the Hotelling-Downs model⁴, the median voter will be the decisive voter regardless of whether candidates are policy- or office-motivated, given certain assumptions. If voter turnout varies, policy outcomes may be different given a certain utility functions of the politicians.⁵ Second, a change in the voter turnout rate may result in a change in the vote share for the parties running in an election. The policy outcome may then change as a consequence of a party representation effect.⁶ This would be more in line with the Citizen-Candidate model⁷, where political candidates implement their preferred policy if they win a political election.

¹Some countries such as the Scandinavian countries, Australia, Malta and Belgium seem to have a positive trend, but the overall picture shows a negative trend for established democracies for the time period 1945-1999. See (Franklin, 2004, p.11)

²See Lijphart (1997) and Caplan (2008)

³See for example Lutz and Marsh (2007)

⁴See Downs (1956)

⁵Given that parties are office motivated they will react to a different position of the median voter since the median voter will be the decisive voter. This may also be true for policy-motivated candidates given that candidates may credibly commit to policy platforms and implements their suggested policy if elected. Voters are assumed to have singled peaked preferences.(Downs, 1956, p. 28-31, 118). See also Roemer (1997) for a discussion.

⁶See Pettersson-Lidbom (2008) and Tyrefors Hinnerich (2008)

⁷See Osborne and Slivinski (1996) and Besley and Coate (1997)

Empirically, the main challenge is to identify the causal effect of voter turnout on policy outcomes. For instance, a two-way causality is possible where voter turnout affects policy outcomes while a certain public policy may simultaneously influence whether people go to the polls. In this paper, I will use an institutional reform as an instrument for voter turnout in order to estimate the effect of turnout on policy outcomes. The empirical strategy involves using Swedish and Finnish municipal data and then exploiting the fact that Sweden changed its constitution in 1970 as an instrument for voter turnout.⁸ This constitutional change reduced the cost of voting in Swedish local elections and, according to economic theory, would result in an increase in the voter turnout rate. Finnish municipalities will act as a control group and I will argue that Finnish and Swedish municipalities are alike and act in similar constitutional settings. In Finland however, local elections and national elections are held on separate days at two year intervals, similar to the system in Sweden before 1970.

By investigating the causal link between voter turnout and the size of government, we may gain insight into the policy implications from the observed decline in voter turnout that many democratic countries have experienced. Does a variation in turnout actually change policy outcomes? In this paper, I find support for this theoretical cogitation. More specifically, the constitutional change in Sweden in 1970 did increase voter turnout. The increase in turnout also resulted in higher municipal taxes and larger local public expenditures. In addition, there seems to be some evidence that political parties belonging to the right-wing block were disfavored when voter turnout was increased.

2 Related literature

In the field of political economics, the causal link between voter turnout and policy outcomes has generally been investigated using cross-country approaches. These results are interesting, but the question remains as to whether the causal link has been identified. Other papers have addressed the matter by studying the extension of the franchise. My paper contributes by combining features that together constitute a better setting for capturing the causal mechanism. First of all, I make use of an institutional change to get exogenous variation in turnout, which enables me to estimate the causal effect of turnout on policy outcomes more convincingly in a second stage. Second, municipal data from Finland and Sweden are very suitable due to the high degree of similarity between the Finnish and the Swedish political

⁸Regarding voter turnout rate with and without a common election day, [Oscarsson et al. \(2001\)](#) has investigated the matter by studying Sweden and other comparative countries and concludes that a common election day seems to be one factor the government may use if they want to increase voter turnout.

systems. As for the matter of external validity, it seems reasonable that the result may be generalizable given that tax rate and public expenditures are universal outcome variables.⁹ Lastly, the first stage analysis of whether a change in the cost associated with voting will influence voter turnout is interesting in itself as it addresses the public choice question whether the cost associated with voting will affect the choice to participate.

Let us begin by reviewing those papers that have an emphasis on the extension of the right to vote. [Lott and Kenny \(1999\)](#) focus on the extension of the voting franchise in the U.S. during the 19th and 20th centuries. More specifically, they investigate the women's right to vote and find that the extension of the franchise resulted in more liberal policies and an increase in public spending. According to [Lott and Kenny \(1999\)](#), there is a gender gap between the way that men and women vote, one potential explanation being that women are more risk-averse than men. According to the authors, women's fears of being left alone as sole breadwinners with the responsibility of raising children creates a higher demand for higher public spending ([Lott and Kenny, 1999, p.1188](#)).

[Husted and Kenny \(1997\)](#) focus on the abolition of literacy test and poll taxes in certain U.S. states during the 20th century. According to [Husted and Kenny \(1997\)](#) The U.S. Voting Rights Acts of the 1960s and 1970s resulted in an increased turnout rate, especially among poor groups in the American society. This extension of the voting franchise resulted in larger redistribution and greater welfare spending. ([Husted and Kenny, 1997, p.79](#)).

The results from the two papers above indicate that policy outcomes will change when the electorate encompasses a larger share of the adult inhabitants. Other papers have instead focused on countries that had a full extension of the franchise, but where the voter turnout rates vary. An increase in voter turnout may be interpreted as a *de facto* enlargement of the voting collective which may yield a similar effect on public policy as an extension of the franchise. Additionally, the actual extension of the voting franchise may in turn be endogenous in itself, which was the conclusion of [Acemoglu and Robinson \(2000\)](#).

[Mueller and Stratmann \(2003\)](#) use a cross-country approach to investigate whether lower turnout will have an effect on economic growth, income inequality and public sector growth. They conclude that a higher voter turnout rate is associated with a larger public sector. Furthermore, they find support for what the authors denote as a *class bias* where lower turnout rate leads to more unequal income distribution. Among other methods, they use an instrumental variable approach in order to address the probable endogeneity of voter turnout. [Mueller and Murrell \(1986\)](#) also apply cross-

⁹The question remains as to whether the results may be generalized to countries where the voter turnout rate is initially lower than in Finland and Sweden.

country analysis. Their focus is mainly on the link between interest groups and the size of government; however, they also find that the turnout rate and population will have a positive impact on the magnitude of the public sector. The idea of class bias is also investigated in [Hill and Leighley \(1992\)](#) who use data from American states. They conclude that the underrepresentation of the poor will result in class bias. For example, they conclude that welfare spending is lower in states where voter turnout among the poor is low.

[Fumagalli and Narciso \(2011\)](#) use the same data set as [Persson and Tabellini \(2005\)](#) in their cross-country study but argue that the voter turnout rate is the transitional variable between the constitution and the economic outcome. [Persson \(2003\)](#) does not study voter turnout but rather the economic effects of constitutions by applying a cross country approach and finds, for instance, that a majoritarian voting system is associated with narrower spending focused on certain groups of marginal voters whereas a proportional voting system is associated with broader spending directed towards all groups in a society. All of these studies rely on cross-country analysis and the causal interpretation of these results may be questioned. The problem with a cross-country method is that countries are diverse by nature and it is difficult to control for all differences between them. Most likely you will have omitted variables resulting in biased estimates.

Let us now switch our focus to studies using more disaggregated data. [Fujiwara \(2010\)](#) examines a voting reform in Brazil entailing the introduction of electronic voting. The reform resulted in a 10 percentage increase in the share of valid votes cast. Voting is mandatory in Brazil, therefore the reform did not increase the turnout rate; instead it augmented the share of valid votes from people that were illiterate. The result was that left-wing parties increased their vote share and that policy outcomes changed, for example public expenditures on healthcare increased. ([Fujiwara, 2010](#), p. 38-39)

[Fowler \(2013\)](#) employs Australian data and focuses on the implementation of mandatory voting. When voting became mandatory, working-class citizen increased their share in the electorate resulting in more votes for the Labor Party. Furthermore, the implementation of mandatory voting laws increased pension spending in Australia in comparison to other OECD countries. ([Fowler, 2013](#), p. 159-160)

[Horiuchi and Saito \(2009\)](#) on the other hand apply Japanese municipal data and election day rainfall as an instrument for turnout to address the problem with potential endogeneity of voter turnout. They find that a higher voter turnout rate in a municipality results in higher intergovernmental transfers to that municipality. The authors discuss pre- and post election political incentives and hypothesizes that projects financed by the central government are targeted at those legal entities with a higher political participation rate. The authors propose that elected governments act in this manner to maximize the probability of reelection. See also [Martin \(2003\)](#).

Lastly, variation in voter turnout may also influence the vote share for different political parties and thus indirectly the policy outcome. The above mentioned papers put the voters' preferences in the center. Other papers, however, emphasizes the politicians, as in studies that focus on the effect of party representation on policy. By applying a regression discontinuity approach to Swedish municipal data, [Pettersson-Lidbom \(2008\)](#) shows that party representation on the municipal level affects both policy and economic outcomes. This conclusion is interesting and points towards the conclusion of the Citizen-Candidate model where the preferences of the parties will determine policy outcome. [Lee et al. \(2004\)](#) also find evidence in favor of the Citizen-Candidate model in which elected politicians implement their preferred policy. Elections to the U.S. House of Representatives between two candidates from opposing parties with equal local support do not seem to moderate the pursued policy of the elected representative in comparison to candidates elected in a Democratic or Republican-majority district. ([Lee et al., 2004](#), p. 807)

[Tyrefors Hinnerich \(2008\)](#) applies an RD estimation procedure to Swedish municipal data from 1959-1966 to study the partisan effect on policy outcome. According to [Tyrefors Hinnerich \(2008\)](#) and in line with [Pettersson-Lidbom \(2008\)](#), parties do matter for policy outcomes; interestingly, however, there seems to be a convergence in policy in those municipalities where the right-wing block and the left-wing block are more equally sized ([Tyrefors Hinnerich, 2008](#), p.8).¹⁰

The remainder of my paper is organized as follows: The following section presents a theory for voter participation on the individual level. Later in the same section I will present a simple model of voters' preferences regarding the size of government. Next, the econometric strategy is presented in the identification strategy section followed by a description of the Finnish and Swedish local political system and a description of the data used in the paper. The results are then presented, followed by robustness analysis and a conclusion. Further regression tables may be found in Appendix 3.

3 Theoretical framework

The focus of this paper is the causal link between voter turnout and policy outcomes and we therefore need a theoretical foundation both for the individual choice regarding voting and a model of preferences for the size of government.

In this paper I use a choice model related to the model presented by [Fiorina \(1976\)](#) and described in [Mueller \(2003\)](#) which suggests that we should

¹⁰This conclusion lies somewhere in-between the Citizen-Candidate model and the more classic Hotelling-Downs model where only voter preferences will matter for policy outcome.

view the individual's choice to vote in light of *the expressive voter hypothesis*. This model is an extension of the classic Public Choice model of voter participation.¹¹ Voters in this model obtain utility not only from having a particular political outcome realized, but also from expressing their opinions in an election together with having a degree of *civic duty*. The model consists of the following simple equations

$$P(\text{vote}) = PB + D - C \quad (1)$$

$$D = D' + B \quad (2)$$

P denotes the probability of being the decisive voter in an election, C the cost associated with voting and B the benefit (gain in utility) of having one's preferred policy alternative implemented rather than some other. In this model, D consists of two different variables, namely B which is the benefit of having a certain policy expressed and D' is the "*civic duty part*"¹² of the D -expression. B is in this model important in itself and not just as a part of the PB in the first equation, meaning that voters both get utility from having a specific policy implemented together with a positive utility associated with expressing their opinion regarding this specific policy. (Mueller, 2003, p. 320).¹³ The cost of voting should be understood as the alternative cost of voting – for example lost income during the time you are at the polling station. One may also think of the cost of voting as the cost of acquiring information regarding the election and the choice of political party.

D' might also be interpreted to mean that voting is also an act of social norms and not only an outcome of an individual utility optimization based on the cost and benefit of voting (Mueller, 2003, p. 320). Therefore it might be utility maximizing to vote given that one's utility function incorporates a social norm variable, such as D' . All individual parameters are however influential in the individual voting decision; a decrease in the cost of voting, C , will *ceteris paribus* increase the turnout rate.

Let us further assume that there is some relationship between the *civic duty* portion of the individual voting choice model, D' and the degree of

¹¹The problem with the classic model is above all that it predicts that, given that voters are rational, the turnout rate should be 0.

¹²See Gerber et al. (2008) for an empirical investigation of the relationship between civic duty, social norms and voter turnout.

¹³Note that D is here more explicitly specified than the more residual explanation given in Riker and Ordeshook (1968) where D is just a *taste for voting*.

education¹⁴, such that $D' = f(E_i)$.¹⁵ If more highly educated groups have a higher level of D' , then they will be overrepresented among the voters when the voter turnout rate is less than 100 percent.¹⁶ This implies that there is a difference between the participating and the abstaining voters. The hypothesis is that less educated groups abstain from voting because their net utility gain is negative in the individual voting choice model.¹⁷ If some of the other variables in the individual voting choice model are altered, it should affect the voter turnout rate. For example, if the cost associated with voting decreases, voter turnout rate should increase everything else being equal. The share of highly educated voters V_H will then be lower than before because this would result in an inflow of less educated voters V_L .¹⁸

$$\downarrow C \rightarrow \uparrow \frac{V_L}{V_H + V_L} \quad (3)$$

Let us now turn to the issue of demand for public goods to examine the implications from the arguments above. The assumed utility function for voters and the constraint regarding the provision for public goods are presented below.¹⁹

¹⁴This argument below also holds if we assume a direct relationship between personal income and level of civic duty.

¹⁵See Appendix 1. Here I assume a linear relationship between education and civic duty. One may argue that very highly educated voters at some point experience a decrease in their civic duty because they realize that they have a better knowledge of the society than the people involved in political life. This effect is however not straightforward.

¹⁶I assume here that B in the D expression is equal between high educated and low educated.

¹⁷The argument above regarding the link between higher education and a higher probability of voting is in itself an empirical question. There are a number of papers in the empirical literature in which the authors argue that higher education increases the probability of voting. See for example Sigelman et al. (1985). Some more recent papers have not found this link between education and voter participation. Knack and White (2000) study voter registration, but find that the possibility of election day- registration does not result in a low bias regarding the prevalence of highly educated groups in the potential electorate. Solis (2012) argues that the long-accepted positive relationship between education and turnout is a spurious correlation. In this paper, the focus is not on this intermediate step, but rather on the effect of a variation in turnout on policy.

¹⁸This may be compared to the Meltzer and Richard (1981) model in which the median voter will be the decisive voter in a country with a democratic voting rule. According to this model, the decisive voter will be equal to the person with the median income which is in turn equal to the person with the median productivity in a society.

¹⁹The model is based on the simple model of public finance presented by (Persson and Tabellini, 2002, p.48-49)

3.1 Voters' utility schemes

$$U_i = c_i + q(G) \quad (4)$$

$$c_i = (1 - t)w_i \quad (5)$$

$$t \in [0, 1] \quad (6)$$

$$W = \sum_{i=0}^{\infty} w_i \quad (7)$$

$$G = tW \quad (8)$$

Voters are assumed to have two sources of utility. First, they get utility from private goods consumption, c_i . Second, they get utility from the provision of public goods according to some concave function $q(G)$. Hence, voters' utility function is quasilinear. To consume private goods, voters require an income and the level of private good consumption is therefore a function of the individual wage w_i . The government taxes voters in order to finance public goods consumption, which is G , and the tax rate is proportional.²⁰ The government must balance its budget and the total amount of public good provision must therefore be equal to the total wage level in the society multiplied by the tax rate. Public goods spending may not be focused on a specific group, i.e., it is a pure public good. Second, the government taxes all individuals with the same tax rate, t (Persson and Tabellini, 2002, p.48-49). The utility function for the voter may then be rewritten as:

$$U_i = (1 - t)w_i + q(G) \quad (9)$$

$$U_i = w_i - \frac{Gw_i}{W} + q(G) \quad (10)$$

Below I define some properties of the utility function. The first order condition of the utility function with respect to G is

$$\frac{\partial U_i}{\partial G} = -\frac{w_i}{W} + q'(G) = 0 \quad (11)$$

$$G_i = q_G^{-1}\left(\frac{w_i}{W}\right) \equiv G\left(\frac{w_i}{W}\right) \quad (12)$$

If the individual wage level as a share of the total wage level, $\frac{w_i}{W}$, will increase, then the marginal utility from public goods provision will decrease. In conclusion, the demanded level of public goods will depend on the wage vis à vis the mean wage level in society. Public goods provision therefore has a redistributive aspect following from the fact that voters have a quasilinear utility function. If you already have a relatively high private good consumption as a result of a higher relative wage level, your demand for

²⁰On the municipal level, tax rates are proportional both in Finland and in Sweden.

public goods provision will decrease. This is because individuals are net contributors to the financing of public goods (Persson and Tabellini, 2002, p.48-49).

If parties react to incentives in accordance with the Hotelling-Downs model, they will reposition themselves in accordance with the position of the median voter.²¹ Given the argument that voters with a higher personal income level (a longer education) have a higher probability of voting and that parties may commit to policy platforms, a lower voter turnout rate is associated with lower taxes and lower public expenditures.²²

The Citizen-Candidate model on the other hand predicts that political candidates implement their preferred policy if elected. A lower voter turnout rate would in this case alter the vote share for certain political parties. Instead of altering the position of political parties, votes are driven towards those parties whose policy platform consists of more redistribution and therefore higher taxes when turnout is increased. It is uncontroversial to assume that this equal left-wing parties. The bottom line is that voter turnout will have an influence on policy regardless of whether we believe in the Hotelling-Downs model or the Citizen-Candidate model. The purpose of this paper is not to evaluate which of these model that has the best predictions, but rather to investigate the link between voter turnout and policy outcomes which is related to both of these models. My suggestion as to how this might be accomplished is presented in the next section.

4 Identification strategy

We are likely to have a problem with two-way causality between voter turnout and policy outcomes. Given the purpose of this paper and the discussion in the theoretical section, the ideal experiment would be to randomize cost of voting in many legal entities within the same country and then estimate the causal effect of a variation in turnout on policy outcome.²³ Because this is not possible, one solution would be to use an instrument for exogenous variation of the cost for voting and then estimate the effect of turnout on policy outcomes in the second stage. In order to apply this empirical strategy, a suitable control group similar to the treatment group is

²¹This is under the assumption of single peaked preferences among the voters. Convergence towards the median voter's position may also be the case, given certain assumptions, if parties are policy motivated instead of office motivated. See Wittman (1973), Wittman (1976), Duggan and Fey (2005) and Roemer (1997).

²²In reality, the actual voter turnout rate is not known to the running parties before the election. Policy platforms, however, are announced before the election. I assume here that parties base their policy platforms on an approximation of the expected voter turnout rate which is grounded on the information of voter turnout rates in previous elections.

²³This is in line with the theoretical model where the cost of voting is related to voter turnout.

needed.

In this paper, Finnish and Swedish municipal data will be used. Sweden changed its constitution in 1970 resulting in a number of new features in the Swedish election system. The constitutional reform affected both the central government and the local government and throughout this paper I will consider this change in the Swedish constitution in 1970 as a reform package and use this as an instrument for turnout.

To begin with, a common election day for parliamentary, county and municipal elections was introduced and the previous four year mandate period was replaced by a new three year mandate period (Oscarsson et al., 2001, p.31). Before the reform, Sweden held elections every second year, with county and municipal elections held together in one year and a parliamentary election held separately two years later. The mandate period was four years for all three levels of government.

Additionally, the bicameral parliamentary system was abolished and Sweden introduced a unicameral parliamentary system. Before 1970, direct elections were held for the second chamber and indirect elections to the first chamber through the county councils (Oscarsson et al., 2001, p. 21, 25, 28-29).

Parallel to the constitutional change regarding the election system, a municipal merger reform took place. In 1966, Sweden had approximately 900 municipalities and in 1974, after the merger reform was completed, 278 municipalities remained. The foremost reason for reducing the number of municipalities was the fact that many municipalities were very small in terms of population. Higher demands on municipal ability to provide a variety of services and a need for each municipality to be functionally independent and able to manage itself within the municipal borders were also important arguments for the municipal merger reform. (Erlingsson et al., 2010, p.15)

The constitutional change that took place in 1970 may be characterized as parts of the individual voting choice model. The introduction of a common election day led to a lowering of the costs associated with voting in local elections. According to the theoretical model, a lower cost associated with voting should increase turnout. At the same time, the municipal merger reform resulted in larger municipalities so that the chance of being the decisive voter in an election was reduced after the merging of municipalities which should then have led to a lower voter turnout in turnout. However, the chance of being the decisive voter even before the merger reform was extremely small. Likewise, the introduction of a unicameral parliamentary system should increase voter turnout because it enhances the importance of voting in a parliamentary election.²⁴ The effect of the introduction of a three year mandate period is more difficult to categorize as a positive or negative

²⁴The parliament has now one legislative body that is directly elected by the people. Prior to 1970, only one of two chambers of parliament was directly elected.

factor for voter turnout. One may think that it is less important to vote because the voters get a new chance every three years. However, the voters may suspect that reforms are implemented faster with a shorter mandate period and that it is therefore more important to vote. The different reforms and their expected effects on turnout are summarized in the table below. I discuss the identifying assumption of the first stage IV-analysis with regard to these expected signs in section 5.1

Reforms	Expected sign
Common election day	++
3 year mandate period	+/-
Unicameral parliamentary system	+
Municipal merger	-

This paper will use instrumental variable regression to estimate the causal effect of voter turnout on policy outcomes. OLS estimates would most likely be inconsistent as a result of two way causality between turnout and policy. The first stage in the IV-analysis consists of a difference-in-difference regression with a binary treatment variable for the Swedish municipalities and a treatment period from 1971. The control group is the Finnish municipalities. In the second stage analysis, I will regress the instrumented turnout variable on policy outcomes. Both Sweden and Finland apply the same election schedule whereby elections are held in the fall of each election year and the newly-elected councils meet in the beginning of the following year.²⁵ The regression equations are thus expressed as:

$$Y_{i,t} = \beta_0 + \beta_1 Turnout_{i,t} + \beta_2 W_{i,t} + \tau_t + f_i + u_{i,t} \quad (13)$$

$$Turnout_{i,t} = \pi_{i,t} + \pi_1 Z_{i,t} + \pi_2 W_{i,t} + \tau_t + f_i + u_{i,t} \quad (14)$$

Y_i denotes the dependent variable of interest. In total, I will have three dependent variables: the municipal tax rate²⁶, total public expenditures²⁷ and vote share for the right-wing bloc.²⁸ β_0 is the intercept. β_1 is the parameter of interest which estimates the effect of a variation in turnout on the dependent variable. The fixed effects are denoted as τ_t and f_i respectively. $u_{i,t}$ is the error term.

²⁵This will be important in my case because municipal mergers took place 1969-1974 in Sweden. In the local election of 1970, for example, people voted for the municipal councils that were legally in place in January 1971, at which time there were approximately 100 fewer than the total number of active municipalities in 1970. Therefore, data regarding turnout in the 1970 election will be merged with municipal finance statistics for the year 1971 and so on. This is done for the entire data panel. Election result may only have an effect on policy after the new councils are in session.

²⁶Denoted *utdebitering per skattekrone* in the Swedish printed statistics and *skatteörets värde* in the Finnish statistics.

²⁷Denoted *summa utgifter* in the Finnish printed statistics 1967-1972 and *egentliga utgifter* between 1972 and 1977 and *utgifter total* in the Swedish printed statistics.

²⁸See section 6 for more details.

Equation (13) thus denotes the second stage in the IV-model. $W_{i,t}$ is a vector of control variables. Both time and municipal fixed effects will be applied in the regression analysis to control for unobservable factors that are constant between entities or over time. In the first stage equation (14), we have a difference-in-difference setup. Variable $Z_{i,t}$ takes the value 1 if the observation belongs to the treatment group (Swedish municipalities) and the treatment period (any year after 1970).

The municipal mergers that took place in Sweden between 1969 and 1974 are of particular concern because these mergers are most likely related to policy outcome. Tyrefors Hinnerich (2009) studies this merger reform in Sweden and finds evidence that municipalities that were going to merge would free-ride and accumulate fiscal debt the years before the aforementioned merger. Jordahl and Liang (2010) focus on the earlier merger reform in Sweden in the 1950's and find that municipalities that were going to merge accumulated new debt for four years prior to the merger. I will address these merger effects in a number of ways. First, Swedish municipalities affected by the merger received a so-called transitional grant to avoid sudden change in the municipal tax rate. These transitional grants are included in the total state grants variable for the Swedish municipalities. The vector of covariates includes a number of interaction variables, in order to control for potential merger effects in the Swedish subsample.²⁹ The values have been deflated and are expressed in USD for relevant variables.³⁰ The covariates used in the analysis are population, state grants³¹ and tax base³², together with merging dummies and dummies for newly created municipalities as well as interaction terms.

Voter turnout is assumed to be constant during a mandate period in the baseline specification, meaning that the turnout rate in a municipality will take the same value for the years up until the next election. This may be problematic due to the increase in the number of included observations where there is no any actual variation in the data. Policy outcomes may however be affected by the turnout rate with a lag and municipal councils may, for example, change the tax rate several times during a mandate period. The

²⁹To start with, I will create dummy variables taking the value 1 if a municipality was merged with another municipality in a given year. Second, I will create a dummy variable indicating whether the observation belongs to a municipality that was newly created in a given year. These dummy variables will be interacted with tax base and population in order to control for the effects of a sudden increase in the number of inhabitants and the tax base due to merger.

³⁰First, I express the nominal values in USD. Then I use a price index based on CPI, with 2005 as base year, in order to deflate the nominal values into real values.

³¹Denoted as *skatteutjämningsbidrag* in the Swedish statistics and *statsbidrag och ersättning* and *summa inkomster av staten* in the Finnish statistics that are divided into rural municipalities and towns.

³²*Antal skatte kronor* in the Swedish statistics and *Antal skatteören* in the Finnish statistics. In my judgment, these are the best corresponding variables for tax base in the Finnish and the Swedish data.

treatment of turnout may therefore have several effects during a mandate period. However, in the robustness section the years in which no election took place will be dropped and the econometric analysis redone. This will be performed with means for all included variables within a mandate period and a mean only for the dependent variable, but where all independent variables are expressed in their yearly values.³³

4.1 Standard errors

Another econometric obstacle is the estimation of the standard errors. Swedish and Finnish municipal data are probably correlated within groups where each municipality cannot be considered a random observation independent of other observations. This concern was first addressed by Moulton (1986) who concludes that if there is some within-group correlation the estimated standard errors will be down-ward biased as a result of a correlation in the error terms. This is often denoted as the Moulton-problem and may result in false statistical significance of point estimates.

One solution is to cluster the standard errors on some appropriate level. One may think that the country level would be appropriate because all municipalities in Sweden and Finland will be correlated to some degree in addition to the fact that the treatment used as instrument was implemented on the national level. The problem then is that I would only have two clusters which are not enough for correct asymptotic properties.

I chose two different strategies in order to address the concerns regarding the estimation of the standard errors. In both Finland and Sweden, municipalities are grouped together in counties³⁴. The counties constituted the central government on the regional level. In Sweden, direct political elections are held for the county councils³⁵, but there are no such elections in Finland. The government appointed a representative, a *landshövding*, in each county, in both Finland and Sweden. Some of the responsibilities of the Swedish *landsting*, such as hospitals, are placed on the municipal level in Finland.

In the Swedish subsample, it is possible that there are clusters of turnout at the county level for various reasons. For example, regional policy may induce whether one casts a vote for the county councils. Because municipal elections are conducted at the same time, this may also affect voter turnout at the local level. However, there are other possible correlation effects that may be present in both the Finnish and Swedish subsample because the counties are responsible for implementing government policy on the local

³³This strategy was chosen due to my inability to control for merging effects if taking means for all included variables because some municipalities did merge during a mandate period.

³⁴Län in Swedish.

³⁵Landstingsfullmäktige in Swedish.

level. Furthermore, the counties in both Finland and Sweden were created in the 17th century and may in some sense be considered as legal entities for a regional structure of socio-economic characteristics that may have an effect on voter turnout. The instrument in this paper is the constitutional change in Sweden in 1970. It is possible that the treatment effect is clustered on the county level as a result of similar political history within a county. Altogether, this may have an impact on voter turnout.

In all, clustering at the county level leads to 34 clusters. One may argue that this is too few.³⁶ Additionally, the constitutional change in Sweden in 1970 was a national reform and I therefore only have one treatment group and one control group, however 10 years in total in my panel. To further address the standard errors issue, I will also estimate standard errors using the approach suggested in Donald and Lang (2007). Briefly, this is a two-step procedure by which data is aggregated for each different group and time combination³⁷, thus reducing the number of observations by collapsing the data. This Donald and Lang specification will be used for the first stage IV and the reduced form specifications, which are the estimations where the binary instrument is directly applied. Formally:

$$Y_{i,t} = \beta_0 + \beta_1 W_{i,t} + \gamma_2 Sweden_i * year_t + \gamma_3 Finland_i * year_t + \tau_t + f_i + u_{i,t} \quad (15)$$

$$\hat{\gamma}_{i,t} = \beta_0 + \beta_1 X_{i,t} + \beta_2 Sweden_i + \beta_3 Finland_i + \beta_4 year_t + u_{i,t} \quad (16)$$

$\hat{\gamma}_{i,t}$ constitutes the predicted values from the first step (covariate adjusted group means) in the Donald and Lang procedure. I use the number of observation in each group and year as weights and estimate equation (16) by weighted least squares (WLS). $W_{i,t}$ is the same vector of covariates used in other specifications in the paper. β_1 is the parameter of interest and $X_{i,t}$ is the binary instrument taking the value 1 if the observation belongs to Sweden and any year after 1970. τ_t and f_i are municipal and year fixed effects. In equation (15), $year_t$ and $Sweden_i$ and $Finland_i$ country dummies are interacted with each other resulting in one binary variable for each time and group combination. By collapsing the data, we end up with two observations from each year – one for the Swedish subsample and one for the Finnish. In the second step (16) I use the saved predicted values to run a regression where I include the variable of interest together with dummy variables for Sweden and Finland and dummy variables for each of the years in my panel.

In sum, both these methods yield more conservative standard errors than ordinary robust standard errors which only compensate for heteroskedasticity in the residuals. Let us now turn to a description of the Swedish and Finnish institutional settings and a discussion of the identifying assumptions.

³⁶In a humorous reference to Douglas Adam's novel *The Hitchhiker's Guide to the Galaxy* Angrist and Pischke (2008) suggest that you should at least have 42 clusters. The number of clusters needed remains under debate.

³⁷The national level in my case and the years 1967-1977.

5 Institutional setting - Sweden and Finland

This paper is based on a similar identification strategy to that in [Dahlberg and Mörk \(2011\)](#). Sweden and Finland have a long common history, and their political institutions display a high degree of similarity.³⁸ The focus in this paper is on local governance and I will argue that Swedish and Finnish municipalities constitute a suitable testing ground for empirical work in public and political economics due to the fact that they are highly independent and exist in two similar institutional environments.

Swedish and Finnish municipalities have the right to collect taxes and they are free to choose their own tax rate. The municipal tax is one of the primary income sources for the municipalities and they may borrow money on the financial market. They receive grants from the central government and provide public services such as social assistance, elderly care and child care and as a result they are fundamental welfare suppliers in each country. Between 1967-1977, both Finland and Sweden were divided into counties, or *län*, in which the so called *länsstyrelse* was the central government's representative in each county. In Sweden, there is also a regional political structure within the same borders as the counties called *landsting*. Political elections are held to the *landstingsfullmäktige* whose prime responsibility is health care. In Finland, health care is the responsibility of the municipalities, but smaller municipalities tend to cooperate over health care.

Both Finland and Sweden are sparsely populated where the inhabitants are clustered in a number of larger cities. The northern parts of each country are even more sparsely populated than the southern parts. As you can see in the graphs displayed in the robustness analysis section, a few municipalities have much higher public spending than the majority of the municipalities.

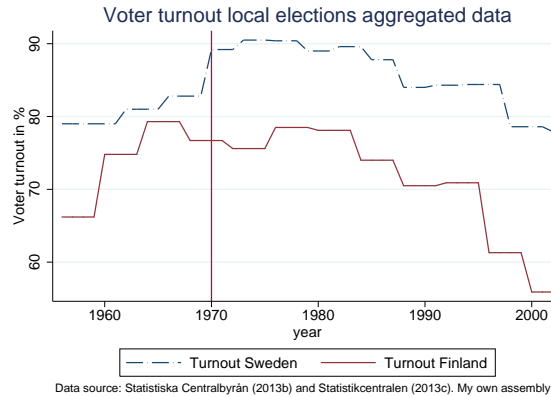
Direct political elections are conducted to fill municipal council seats every fourth year (in Finland and in Sweden before 1970) and each third year (in Sweden after 1970). Both countries conduct elections through a PR voting system.³⁹

My first stage uses a difference-in-difference approach and the main identifying assumption behind DiD estimation is that of parallel trends in the variable of interest. Swedish municipalities should have a parallel trend in turnout in comparison to the Finnish municipalities and the development in turnout rate should look the same if the Swedish municipalities had not experienced the constitutional change in 1970. I will present a graph below illustrating the average turnout rate in local elections for Swedish and Finnish municipalities. As you can see, the average voter turnout rate is higher in Sweden for the entire time period, but there is an increase in 1970 for the Swedish subsample. After 1970, the trend in each country is also sim-

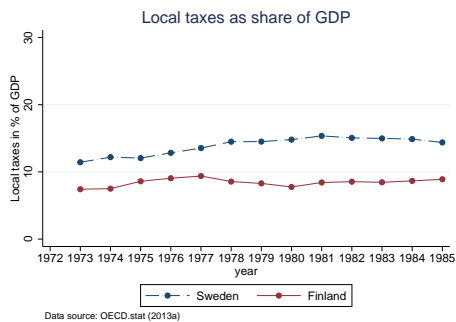
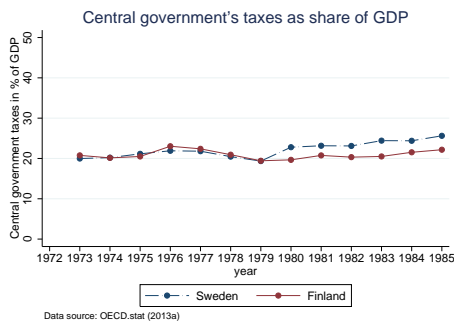
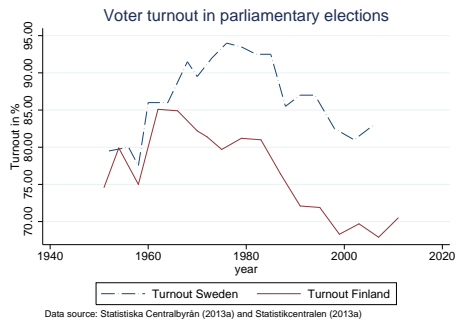
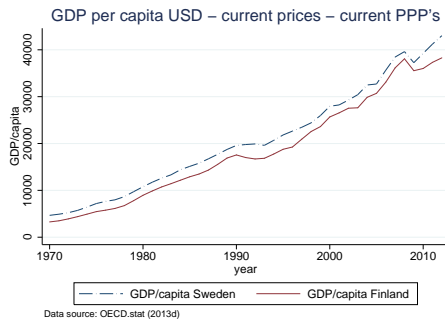
³⁸Finland was a part of Sweden from the early middle ages up until 1808.

³⁹See [Petterson-Lidbom \(2012\)](#) and ([Dahlberg and Mörk, 2011, p.482-484](#)) for a description of Swedish and Finnish local governments.

ilar, but the difference in turnout rate is larger. Note that voter turnout is displayed as constant during a mandate period in this graph. The conclusion is that Finland and Sweden have a similar trend in this variable.



One important assumption in this paper is that Finland and Sweden are similar countries. In addition to the description of the responsibility of the local sector above, I show some figures below displaying voter turnout in parliamentary elections, GDP per capita, the central government's taxation in percentage of GDP and local taxes as percentage GDP. For some of these variables I only have access to data for a shorter time period.



5.1 Threats to identification

Regarding instrumental variable regression, we need instrument exogeneity. Is there reason to believe that the instrument should affect policy outcomes directly and not through the turnout variable? The 1970 constitutional reform in Sweden was decided by the central government and the policy variables in focus in this study are in the legal jurisdiction of the municipalities. If an effect is present between the constitutional change and the policy outcomes on the municipal level, then it must be an indirect effect.

In the years prior to the constitutional reform, a public constitutional inquiry had taken place. When this inquiry was presented, none of the political parties in the Swedish parliament were in favor of the idea of a common election day. The choice of a common election day was instead the result of a compromise as it was considered vital that all political parties unanimously agreed on the constitutional change. In fact, it was the issue of the single chamber parliamentary system that divided the political parties. The Social Democrats wanted to keep the bi-cameral system and the right-wing parties supported a unicameral parliament. The upper chamber had a local connection since its members were elected indirectly through the county councils and the Social Democrats argued that the local connection in national politics would be lost if the upper house was abolished. The center-right parties, however, ultimately prevailed against the two-chamber parliamentary system. As a compromise, a common election day was introduced and the two-house parliament was replaced by a single chamber parliament. Because all elections were grouped together, there was still some local connection in the national election in accordance with the compromise. (Oscarsson et al., 2001, p.29-31).

Historical records show that the outcome of the constitutional change in Sweden was largely due to political logrolling on the national level. The municipalities were undeniably affected by these reforms, but it is difficult to imagine why they should affect policies such as tax rates and public spending directly because many of the decisions were made over the heads of local politicians. Swedish municipalities have a high degree of independence and they may set public policy without consulting with the central government. Regarding the new, and shorter, 3 year mandate period that was introduced at the same time, it is, in some sense, easier to argue that this reform could affect the municipal policy outcome. There is however no clear-cut theoretical prediction as to what we should expect from such a reform.

In conclusion, there is no particular indication that the constitutional reform should have affected the policy outcomes in the Swedish municipalities and as a consequence, no obvious reason to believe that we have a threat against the assumption of instrument exogeneity.

Another threat against identification is that the monotonicity assumption of the first stage is not fulfilled. Formally, we need $w_1 - w_0 \geq 0 \forall i$,

where w is the binary indicator for the DiD instrument in the first stage. In essence, implementing the constitutional reform in Sweden cannot decrease voter turnout in some municipalities. To examine this, I will rerun my first stage analysis for different subsamples: One group with municipalities that were merged and one group with municipalities that were not merged, and two other regression specifications where highly populated and less populated municipalities are analyzed separately. The results will be presented in the robustness analysis section.

6 Data

The data were collected from Statistics Sweden and Statistics Finland, from the publication series *Årsbok för Sveriges kommuner, Kommunal Finansstatistik, Årsbok för Finland, Statistisk Rapport, Allmänna valen* and *Kommunalvalen*.⁴⁰ Some of the data have been downloaded in digital format; however the data are not available in digital form for the majority of the years covered and the variables used. Data has therefore been converted into a digital format using Optical Character Recognition (OCR).⁴¹ Please see the section after the References list named *Printed data sources* for a full list of the printed statistics publications which are used in the paper. Electronic data sources with URL-links may be found in the section just below.⁴² Descriptive statistics for a selection of variables is presented below.⁴³ The municipalities of Stockholm, Malmö and Göteborg have been excluded from the analysis, together with the municipalities of Åland and Gotland because these particular municipalities have had different responsibilities than the rest of the municipalities included in the sample for some years. The three dependent variables in the empirical analysis are municipal tax rate, total public expenditures and vote share for the right-wing block. The variable of interest

⁴⁰The Government Institute of Economic Research (VATT) has provided data regarding mergers of Finnish municipalities. Statistics Sweden has provided data regarding Swedish municipal mergers. Data regarding CPI, GDP, exchange rates and aggregated measures for taxation as share of GDP comes from OECD Stat.

⁴¹The OCR process is an efficient process for converting large paper-based data sets into digital format. The process is not without flaws, however, and misinterpretation may occur. Some of these errors are easily spotted and may be corrected directly when performing the econometric analysis. Furthermore, I will perform a sample check of my data in order to examine the prevalence of OCR-error which is presented in Appendix 2. Some remaining misinterpretations still exist in the final data set.

⁴²Election data on the municipal level are available from 1973 in digital format for the Swedish subsample and after 1976 for the Finnish.

⁴³For the public expenditures outcome variable, the statistics from Statistics Sweden is reported with a 2 year lag. Public expenditures for the year of 1973 are printed in the *Årsbok för Sveriges kommuner 1975*. As a result, the sample is somewhat reduced in comparison with the analysis regarding tax rate because some municipalities have over the mentioned two years merged with other municipalities.

is voter turnout and the included covariates are tax base, population⁴⁴ and state grants. In addition, I have balanced the panel so the same number of observations is always present in each specification regardless of which covariates are included.⁴⁵

In the upcoming empirical analysis, I will investigate whether a variation in voter turnout influence the vote shares of the parties running in the election. In Sweden and in Finland, political parties may be divided into a right-wing and a left-wing block and the vote share for one entire block will act as dependent variable. The reason for grouping the data into political blocks is the lack of data for specific parties for the earlier years in the Finnish data set. The time period analyzed is 1967-1977. The right-wing block will consist of the Conservative party, the Christian Democrats, the Center party and the Liberal Peoples Party in the Swedish subsample. The left wing bloc incorporates the vote shares for the Social Democrats and the Left party. For the Finnish subsample, the Conservative party, the Christian Democrats, the Swedish Peoples Party, the Liberal Party and the Center Party will constitute the right wing bloc together with minor right wing parties in accordance with the definition of *Statistikcentralen*. The Finnish left wing block is the Social Democrats, the Social Democratic Union of Workers and Small Farmers and the Democratic League of the People of Finland together with other minor left wing parties.⁴⁶

A sample investigation has been carried out to evaluate the OCR process. See Appendix 2 for details.⁴⁷

⁴⁴Statistics Finland split their statistics series in 1973 for the Finnish municipalities. Before 1972, population was measured yearly on the first of January each year (*mantalsskriven befolkning*), but in the new publication *Statistisk Rapport* the population is measured yearly on December 31st. One solution would be to merge population statistics for Finnish municipalities after 1973 with a one year lead. This results in having no observations for 1973. Therefore, I do not pursue this procedure. The chosen solution is a somewhat problematic, but in my opinion the least bad.

⁴⁵Because my included variables originate from a number of different publications and a large proportion of the data have been OCR-converted, some variables for some municipalities becomes missing observations for various reason when all the different data sets were combined. I have tried to manually compensate for this (dofile may be provided upon request), but some missing values still exist in the final data set.

⁴⁶For the Finnish election in 1976, there are no aggregated measures for the blocks. In this case, a block variable is created. The right-wing bloc will then be the Conservative party, the Swedish People party, the Center party, The Liberal party, the Christian Democrats and the Constitutional Peoples' party. The left wing bloc consists of the Social Democrats and the Left party

⁴⁷The reader should be aware that there are some remaining measurement errors in the final data set. These errors should be random however and thus should not affect the estimates in a high degree. See Appendix 3 for an analysis in which I drop random part of the data and show that the point estimates and the statistical significance are relatively unaffected.

Table 1: Descriptive Statistics, means and standard deviations for the time period 1967-1977

	Finland		Sweden	
	mean	sd	mean	sd
Municipal tax rate	14.63	1.75	12.61	1.78
Turnout	79.21	4.51	86.10	5.22
Number of inhabitants	9507.42	25828.68	12422.41	17886.41
State grants in thousands	28611.33	61798.64	2615.64	4367.99
Taxbase in thousands	503605.55	2336429.91	180435.88	287098.07
Municipal merge during the year	0.01	0.10	0.05	0.21
Public expenditures	139359.75	598880.78	92513.03	151251.34
New municipality during the year	0.00	0.01	0.00	0.05
Vote share right wing-block	60.48	16.82	51.03	17.38
Vote share left wing-block	36.33	15.54	44.09	14.54
Observations	5132		5751	

7 Results

The main results will be presented in this section and additional regression tables may be found in Appendix 3. In the main specifications, results from estimation both with and without municipal and years fixed effects are reported. In later specifications, only estimates with municipal and years fixed effects will be reported because I believe that fixed effects are needed to estimate a more correct model. In column 1 and 2 in the tables below, I do not use the panel dimension in my dataset. The standard errors are not clustered in the first column in the tables below, but are for the remaining columns as well as alternative specifications after the main results. I cluster on the county level. I choose this strategy to be as transparent as possible.

I will begin by examining the OLS regression outputs treating turnout as an exogenous variable. Table 1 shows that turnout is statistically significant in the first simple regression case and that the point estimate is negative. Because we believe that fixed effects are needed, this result is rather uninformative. The estimated correlation is positive when municipal fixed effects are included but the statistical significance drops to the 10 % level when both municipal and year fixed effects are added. The additional control variables group consists of variables for the tax base, number of inhabitants and state grants as well as interaction variables for 1) municipal merge and population and 2) municipal merge and tax base. Because turnout is most likely an endogenous variable, the IV-specifications are more adequate and the point estimates in the OLS specifications are most likely biased. Therefore, it is not meaningful to analyze the economic significance of these OLS-estimates, but we may conclude that the estimated correlations are positive when fixed

effects are added. Hence, let us continue to the first stage IV-estimation.

Table 2: OLS estimates

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Taxrate	Taxrate	Taxrate	Taxrate	Taxrate
Turnout	-0.042*** (0.003)	-0.013 (0.017)	0.166*** (0.030)	0.019* (0.010)	0.017* (0.009)
Municipal merge during the year		-0.113 (0.215)			-0.088 (0.059)
New municipality during the year		-0.303 (0.375)			0.300* (0.148)
Observations	10,907	10,907	10,907	10,907	10,907
R-squared	0.016	0.168	0.145	0.787	0.789
Clustered standard errors?	No	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	No	Yes
Municipal fixed effects?	No	No	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes
Number of Municipalities			1,446	1,446	1,446

Normal or clustered standard errors respectively in parentheses

*** p<0.01 ** p<0.05 * p<0.1

To begin with, table 3 indicates a strong first stage where the variable of interest, Constitutional change 1970, is statistically significant and the estimated parameter value is large. Note that the variable Constitutional change 1970 is an interaction variable between the variables Treatment group (equals 1 if the observation belongs to the Swedish subsample) and Treatment period (equals 1 if the observations belong to any year after 1970). The conclusion is that the reform package introduced in Sweden in 1970 did have an effect on voter participation. The estimated effect is robust for all specifications. This result is interesting and indicates that voter turnout will increase when the cost of voting is reduced. We may also conclude that we have a strong first stage by looking at the F-statistics from the first stage. In all specifications, the F-value exceeds the rule-of-thumb value of 10 and we may therefore conclude that the identifying assumption of instrument relevance is fulfilled. The constitutional reform in Sweden seems to increase voter turnout rate by approximately 6 percentage points.

Table 3: First Stage IV-estimates

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Turnout	Turnout	Turnout	Turnout	Turnout
Constitutional change 1970	6.806*** (0.170)	6.728*** (0.501)	6.594*** (0.458)	6.834*** (0.444)	6.533*** (0.443)
Treatment group	3.928*** (0.123)	3.758*** (0.776)			
Treatment Period	-0.538*** (0.125)	-0.320 (0.378)	-0.564* (0.327)	-0.120 (0.304)	1.565*** (0.359)
Municipal merge during the year		0.024 (0.336)			0.047 (0.160)
New municipality during the year		-1.268 (0.769)			-0.534 (0.433)
Observations	10,907	10,907	10,907	10,907	10,907
R-squared	0.472	0.480	0.538	0.583	0.594
Clustered standard errors?	No	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	No	Yes
Municipal fixed effects?	No	No	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes
F-value	1597	180.4	207.5	237.4	217.2
Number of Municipalities			1,446	1,446	1,446

Normal or clustered standard errors respectively in parentheses

*** p<0.01 ** p<0.05 * p<0.1

In table 4, the second stage IV regression outputs are presented. This IV specification should address the potential two-way causality between policy outcomes and voter turnout. The voter turnout variable is statistically significant for all specifications and the estimate parameter value for the variable of interest is positive. There seem to be some negative correlation between a municipal merger and the local tax level. The inclusion of the merging dummy, however, does not seem to alter the point estimate of the turnout variable.

In summary, turnout rate seems have an effect on municipal tax rates. In the full model, the point estimates equal 0.04, which should be interpreted as an increase of 0.04 percentage points in municipal tax rate when voter turnout increases one percentage point. This estimated effect is not enormous, although municipals seldom make drastic changes to municipal tax rates. If we consider the reduced form estimates presented in Appendix 3, which are equal to 0.258 in the fully specified model, the constitutional reform in Sweden increased the tax rate through voter turnout by 0.258 percentage points. This increase constitutes approximately 6.5% of the total increase in tax rate during the time period for the Swedish subsample. In summary, the estimated effect should be considered economically significant.

Table 4: Second Stage IV-estimates

VARIABLES	(1) Taxrate	(2) Taxrate	(3) Taxrate	(4) Taxrate	(5) Taxrate
Turnout	0.066*** (0.006)	0.119*** (0.033)	0.315*** (0.021)	0.037** (0.016)	0.040** (0.016)
Municipal merge during the year		-0.639*** (0.151)			-0.093 (0.059)
New municipality during the year		-0.552 (0.385)			0.313** (0.152)
Observations	10,907	10,907	10,901	10,901	10,901
Clustered standard errors?	No	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	No	Yes
Municipal fixed effects?	No	No	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes
Number of Municipalities			1,440	1,440	1,440

Normal or clustered standard errors respectively in parentheses

*** p<0.01 ** p<0.05 * p<0.1

Let us turn to public expenditures. When performing the same operation using total public expenditures as dependent variable, as in table 5 below, the results are in line with those discussed above. Here, public expenditures are here expressed in thousands of USD in 2005 prices. Again, the OLS specification is difficult to interpret since the point estimates are fairly variable for different specifications. Once again, however, I may conclude that there seems to be a statistically significant correlation between voter turnout and public expenditures when fixed effects are included together with additional covariates.

When examining the IV specification, the same pattern that was exhibited when tax rate was the dependent variable manifests itself. The magnitude of the point estimates is reduced when year fixed effects are included, but becomes larger after the inclusion of additional covariates. A one percentage points rise in turnout increases public expenditures by approximately 7 000,000 USD in 2005 prices according to the fully specified model. If we relate this to the reduced form estimate in table 13 in Appendix 3, public expenditures increased by 47590,000 USD as a consequence of the reform. This increase constitutes approximately 27 % of the total rise in public expenditures for Swedish municipalities for the time period 1967-1977. The estimated effect should therefore be considered economically significant.

Table 5: OLS estimates

VARIABLES	(1) PubExp	(2) PubExp	(3) PubExp	(4) PubExp	(5) PubExp
Turnout	-5,997*** (769)	789*** (257)	5,613*** (1,316)	1,755** (796)	5,026*** (1,752)
Municipal merge during the year		-17,219*** (4,085)			-2,920 (4,570)
New municipality during the year		-19,586*** (6,704)			-16,740** (7,862)
Observations	9,591	9,591	9,591	9,591	9,591
R-squared	0.006	0.948	0.016	0.071	0.294
Clustered standard errors?	No	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	No	Yes
Municipal fixed effects?	No	No	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes
Number of Municipalities			1,394	1,394	1,394

Normal or clustered standard errors respectively in parentheses

*** p<0.01 ** p<0.05 * p<0.1

Table 6: Second Stage IV-estimates

VARIABLES	(1) PubExp	(2) PubExp	(3) PubExp	(4) PubExp	(5) PubExp
Turnout	5,000*** (1,237)	818*** (255)	9,910*** (1,040)	4,574*** (1,136)	7,000*** (2,089)
Municipal merge during the year		-17,344*** (4,879)			-3,244 (4,588)
New municipality during the year		-19,648*** (6,708)			-15,836* (8,695)
Observations	9,591	9,591	9,571	9,571	9,571
Clustered standard errors?	No	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	No	Yes
Municipal fixed effects?	No	No	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes
Number of Municipalities			1,374	1,374	1,374

Normal or clustered standard errors respectively in parentheses

*** p<0.01 ** p<0.05 * p<0.1

7.1 Party representation effects

As mentioned above, a potential political party effect might be an intermediate factor behind these results. The argument is that voter turnout will affect the share of votes for the various political parties and that the elected politicians will implement their preferred policy in accordance with the Citizen-Candidate model. To investigate this, I use the vote share for the right wing block as the dependent variable and then estimate the effect of turnout on vote share treating turnout as exogenous and endogenous. Note that all specifications in the results section hereafter are specified with

clustered standard errors as well with the inclusion of time and municipal fixed effects.

In the fully specified model, we have no statistically significant results in any specification and the point estimates are positive.⁴⁸

Table 7: First and second stage and reduced form estimation; dependent variable is the vote share in % for the right wing block

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	OLS RW.vote.share	OLS RW.vote.share	IV-Second stage RW.vote.share	IV-Second stage RW.vote.share	Reduced form RW.vote.share	Reduced form RW.vote.share
Turnout	0.006 (0.088)	0.009 (0.090)	0.124 (0.169)	0.116 (0.167)		
Constitutional change 1970					0.908 (1.250)	0.832 (1.214)
Observations	3,701	3,701	3,244	3,244	3,701	3,701
R-squared	0.054	0.061			0.055	0.061
Number of Municipalities	1,439	1,439	982	982	1,439	1,439
Clustered standard errors?	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Covariates?	No	Yes	No	Yes	No	Yes

Clustered standard errors in parentheses

*** p<0.01 ** p<0.05 * p<0.1

The sample used in this case, however, is somewhat problematic. In some municipalities, the right-wing block received no or very few votes. When studying the histogram below, it is clear that there are some distinct outliers in the data that may drive the results presented above. I will therefore rerun the analysis dropping those observations where the right-wing block received less than 1 percent of the votes. I also drop the municipalities where the right-wing bloc received over 99 percent of the votes. The results are presented in the table below the histogram. For this specification, the estimated parameter values are still insignificant, but the point estimates now become negative.

⁴⁸Only election years are studied when analyzing the link between voter turnout and vote shares, so the sample size consequently becomes smaller.

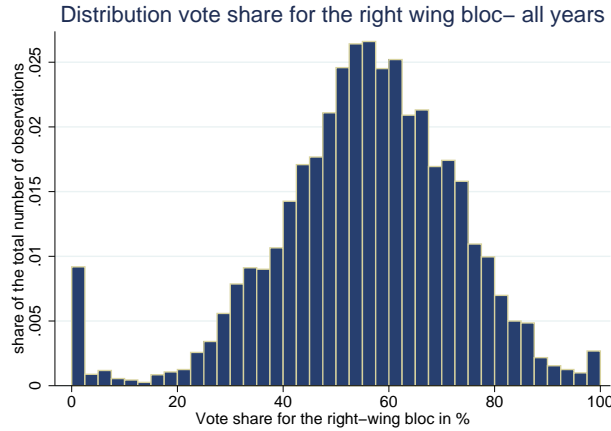


Table 8: First and second stage and reduced form estimation; dependent variable is the vote share in % for the right wing block. Outliers are deleted

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	OLS RW.vote.share	OLS RW.vote.share	IV-Second stage RW.vote.share	IV-Second stage RW.vote.share	Reduced form RW.vote.share	Reduced form RW.vote.share
Turnout	-0.068 (0.060)	-0.099* (0.057)	-0.064 (0.136)	-0.134 (0.120)		
Constitutional change 1970					-0.471 (1.028)	-0.957 (0.884)
Observations	3,623	3,623	3,170	3,170	3,623	3,623
R-squared	0.118	0.126			0.117	0.126
Number of Municipalities	1,414	1,414	961	961	1,414	1,414
Clustered standard errors?	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Covariates?	No	Yes	No	Yes	No	Yes

Clustered standard errors in parentheses

*** p<0.01 ** p<0.05 * p<0.1

In some municipalities, local parties received a large share of the votes for various reasons and these parties are not always easy to categorize as either right-wing or left-wing. For example, they might be single-issue parties. To see if this will affect the results, I also run the same specification as in table 7, but drop those municipalities where local parties received more than 5 percentage points of the votes. This action renders the estimates statistically significant and the point estimates negative and quite larger. This is displayed in table 9. In this specification we have a statistically significant effect for all specifications, both with and without included covariates.

There seems to be some evidence that a higher voter turnout rate is negative for the vote share of right-wing parties when excluding municipalities with powerful local parties. The table below shows that for each percentage point of increase in voter turnout, the vote share for the right-wing block

decreases by over half a percentage point in the second stage IV specification. This effect is rather large and there is, as a result, some evidence that the constitutional change implemented in Sweden in 1970 decreased the vote share for right-wing parties. One must remember that voter turnout is both Sweden and Finland was high during the time period 1967-1977; therefore the estimated effect must be related to a mechanism where turnout increases from a high level to an even higher level.

Table 9: First and second stage and reduced form estimation; dependent variable is the vote share in % for the right wing block. Outliers are deleted

VARIABLES	(1) OLS RW.vote.share	(2) OLS RW.vote.share	(3) IV-Second stage RW.vote.share	(4) IV-Second stage RW.vote.share	(5) Reduced form RW.vote.share	(6) Reduced form RW.vote.share
Turnout	-0.207*** (0.046)	-0.218*** (0.052)	-0.463*** (0.078)	-0.533*** (0.080)		
Constitutional change 1970					-3.439*** (0.560)	-3.828*** (0.526)
Observations	3,262	3,262	2,842	2,842	3,262	3,262
R-squared	0.224	0.234			0.247	0.261
Number of Municipalities	1,312	1,312	892	892	1,312	1,312
Clustered standard errors?	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Covariates?	No	Yes	No	Yes	No	Yes

Clustered standard errors in parentheses

*** p<0.01 ** p<0.05 * p<0.1

In conclusion, there is evidence of a causal link between voter turnout and policy outcomes related to the size of government. There is also some evidence that voter turnout is negatively associated with the vote share for the right-wing block, at least after excluding municipalities with strong local parties. Therefore, we cannot completely rule out that the estimated effect on policy goes through an intermediate variable consisting of the vote share for the political parties. I may, however, conclude that voter turnout is associated with the issue regarding the size of government, here defined as tax rate and public expenditures. The effect may either be through the political parties or as an incentive whereby all political parties change their policy position. I cannot rule out either the Hotelling-Downs model or the Citizen-Candidate model since they are both in line with the results in this paper. Nonetheless, the results are interesting in that they point toward an underlying mechanism that might be important for both of these models, namely voter turnout.

In the section below I continue with some alternative econometric specifications and discuss some additional issues related to the empirical analysis.

8 Robustness analysis

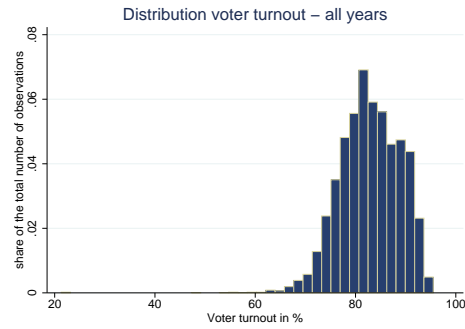
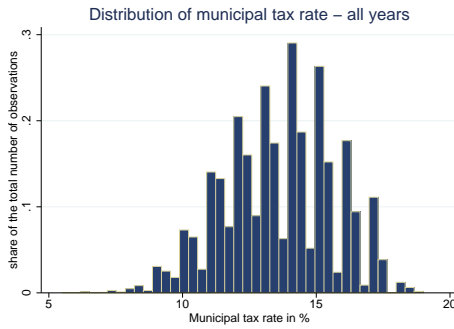
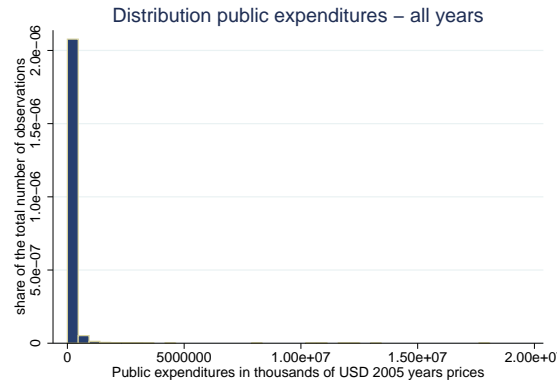
To begin with, a multicollinearity check is performed by calculating the VIF values of the included covariates in the OLS and IV models. The overall conclusion is that there is no serious problem with multicollinearity for the variable of interest.⁴⁹

Because I have converted the data using OCR, there is a potential risk of systematic misinterpretation stemming from the OCR-process. One way to evaluate if the estimation results are driven by a systematic misinterpretation⁵⁰ is to analyze different subsamples of the data and then rerun the analysis. The result for the variable of interest (voter turnout), for different subsamples is presented in Appendix 3. All covariates are included but only the estimated parameter value for turnout is reported. I conclude that this robustness analysis does not seem to indicate the presence of any systematic bias in the data.

I will also check the potential effect of outliers in the data set. Some municipalities have very low turnout rates and it is interesting to see if the previously found effect is driven by these outliers. Because the lion's share of the data regarding turnout is above 60% I will also rerun the analysis excluding those observations belonging to municipalities with a lower voter turnout rate. Furthermore, some municipalities have very high or very low tax rates. I will also delete those observations, namely those with a tax rate below 8 % and above 18 %. The results are presented in table 14 in Appendix 3. The same method applies regarding public expenditures as an outcome variable. Certain municipalities have much higher public expenditures than others. Helsinki, the capital of Finland, is for example included in the dataset, but Stockholm is not because Stockholm has a different administrative structure than other Swedish municipalities. Therefore, I will exclude those municipalities whose total public expenditures exceed 100,000,000 USD and redo the analysis. The results are presented in table 15. Admittedly, these cut-off values are arbitrarily chosen by eyeballing the data, but it is still interesting to see the potential changes in the estimated effects when these outliers are removed. I present the histograms for the municipal tax rate, turnout and public expenditures below. The overall conclusion is that the estimated results are robust to these procedures, but that the magnitude of the point estimates are reduced; especially in table 15 for the public expenditures variable.

⁴⁹For some of the included covariates, we have a VIF value over 5, which may be problematic because it will increase the size of the standard errors resulting in less precise estimation intervals. For the variable of interest, voter turnout, the VIF value is only 1.16, so the standard errors for our variable of interest is not affected, nor are the coefficients for the other covariates.

⁵⁰For example if a certain number, say a 7 has been interpreted as a 1, for some parts of the datafiles.



In the main specification, I use total public expenditures. As a robustness check, I also redo the analysis with public expenditures per capita. The conclusion is that the precision in the point estimate is decreased when further covariates are included, so in the fully specified model, we do not have a statistically significant result for the variable of interest. The estimated coefficient for turnout switch signs for the fourth column, in comparison to table 5 and 6 in both the OLS and IV specification. The results in table 17 and 18 in Appendix 3 are not entirely in line with the main results and seems to be somewhat more dependent whether you include covariates or not. One interpretation might be that municipalities with lower public expenditures per capita also differ in other aspects. Municipal mergers may also play a role where certain municipalities in a given year suddenly become more populated, thus affecting public expenditures per capita.

As already mentioned, voter turnout is treated as constant during a mandate period. One might argue that this choice will increase the number of observations in the data set. As a result, I will redo my econometric analysis, taking the mean of the municipal tax rate and public expenditures for a mandate period. I will also take the mean of the other covariates included

in the analysis. When simply taking the mean of the dependent variable, I may still include merging dummies in the estimation as some municipalities did merge during a mandate period. When taking the mean for all dependent and independent variables, this part of the analysis is left out. In all, four new specifications are presented in Appendix 3. The point estimates remain positive for all specifications, and the statistical significance remains for the specifications with mean tax rate, mean public expenditures and mean public expenditures together with means for all other covariates. For the specification with mean tax rate together with means for all other covariates, the estimated parameter values are no longer statistically significant, but are still positive.

8.1 Monotonicity in the first stage

As already explained, a potential threat to the identification strategy is that the constitutional reform in Sweden negatively affected voter turnout on the local level for some municipalities. Therefore, the sample will be split, and the first stage estimations for those municipalities that were merged and those that were not affected by the merge will be run separately. Then, a similar split between municipalities that are above and below the mean population value will take place. The results of this analysis show that the estimated effect is statistically significant, large and positive for all four specifications, which suggests that there is no evidence that the monotonicity assumption is violated. Please see table 24 in Appendix 3.

8.2 Donald and Lang estimator

In this section, I redo my regression analysis using the Donald and Lang two step estimation technique. In this case, I will focus on a higher administrative level, namely the country level, which is the level the treatment group is defined. This technique aggregates my data to only two observations each year, thus reducing the sample substantially to 22 observations and 8 observations respectively depending on which dependent variable is used. Below I will display the regression results for the first stage IV and the reduced form, both having the binary instrument as variable of interest. The number of observations in each group and year combination is used as weights in order to estimate the WLS.

If we begin with my first stage IV-estimation, I still have a statistically significant result and the point estimate is equal to approximately 6 just as before, meaning that the reform implemented in Sweden in 1970 increased voter turnout by more than six percentage points. In comparison to table 3, in which the baseline first stage results was presented, the standard errors are now larger, however, the F-value is still clearly above the rule of thumb value of 10.

Table 10: Donald and Lang’s estimator, First stage IV

VARIABLES	(1)
	First stage IV Turnout DonaldLang
(mean) instrument1	6.862*** (0.680)
Observations	22
R-squared	0.964
Additional covariates?	Yes
Municipal fixed effects?	Yes
Year fixed effects?	Yes
F-value	101.7

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Let us continue to the reduced form specifications. Regarding tax rate and total public expenditures, the estimated parameter values are still statistically significant. The reader should note that I cluster the standard errors on the county level in the main specification and in this section; I collapse the data on the national level for each year. In the third column in the table below, the same underlying specification as in table 9 is displayed where municipalities with strong local parties have been dropped from the analysis. We do not have a statistically significant effect of the instrument on the vote share of the right-wing block in this specification.

Table 11: Donald and Lang’s estimator; reduced form. Municipal tax rate, public expenditures and vote share in % for the right-wing block

VARIABLES	(1)	(2)	(3)
	Reduced form Taxrate DonaldLang	Reduced form Pub.exp DonaldLang	Reduced form RW.vote.share DonaldLang
(mean) instrument1	0.229** (0.099)	58,504.2** (18,667.2)	-1.955 (3.704)
Observations	22	22	8
R-squared	0.9997	1.0000	0.9996
Additional covariates?	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The conclusion from this procedure is somewhat mixed. When analyzing the first stage and the reduced form we may conclude that the main results seem to hold for the first two variables. As for the vote share for the right wing block, the results are more ambiguous, just as before.

9 Conclusion

In this paper, the causal link between voter turnout and the size of government has been investigated with the overall conclusion that a link does exist between increase in voter turnout and higher tax rates and larger public spending on the local level. Because there is likely a two way causality channel between voter turnout and various policy outcomes, a constitutional reform implemented in 1970 in Sweden was used as an instrument for voter turnout. I found that this reform, which decreased the cost associated with voting, increased the voter turnout rate in local elections, a finding in line with standard Public Choice theory. I also found that a higher voter turnout rate is associated with a lower vote share for right-wing parties, at least after removing municipalities with strong local parties. Given that we have a valid binary instrument and assuming monotonicity of the instrument, the instrumental variable regression estimates the Local Average Treatment effect (LATE). See [Imbens and Angrist \(1994\)](#).

These empirical results are in line with the Hotelling-Downs model and the Citizen- Candidate model. To begin with, these results might suggest that parties react to incentives and reposition themselves when the voter turnout rate varies to maximize their vote share. The results may also be interpreted as evidence of a representation effect whereby political parties implement their preferred policy. In brief, when explaining policy outcomes, the Hotelling-Downs model emphasizes the voters whereas the Citizen-Candidate model focuses on the politicians. This paper, however, points towards another source that influences policy outcomes, namely voter turnout. Regardless of whether the political parties reposition themselves or different political parties gain representation when there is a decline or an increase in voter turnout, participation plays a role in determining which political policies we get.

My results are in line with earlier studies, such as [Mueller and Stratmann \(2003\)](#), [Fujiwara \(2010\)](#) and [Fowler \(2013\)](#) seeing that a higher turnout rate seems to increase the size of government. My estimated effects, however, may also be related to the conclusions in [Pettersson-Lidbom \(2008\)](#), [Lee et al. \(2004\)](#) and [Tyrefors Hinnerich \(2008\)](#) with the addendum that there seems to be some evidence that voter turnout affects political parties' vote share; at least in some of my included specifications.

The welfare state is particularly large in both Finland and Sweden and many welfare services are administered at the local level. The growth of the local public sector was particularly intense in the 1960s and 1970s in Finland and Sweden, and many different factors influenced the expansion of the public sector during this particular time period. Voter turnout seems to be one of these factors, an interesting conclusion that helps us understand some part of the mechanism behind the growth of government. The results from this paper should have some external validity because tax rate and

public expenditures are universal outcome variables. If we believe that the identification strategy consisting of using municipal panel data from Sweden and Finland yields more credible estimates than a cross-country analysis, the conclusions from this paper may hold outside the context of Sweden and Finland. The world has experienced a drop in voter turnout in recent decades, and hopefully the results presented in this paper will be useful when analyzing this declining trend in voter turnout rate. Voter turnout rates might be a factor we sometimes have overlooked when analyzing public policy.

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Appendix 1

Given the voters' utility function, the assumption that $D' = f(E_i)$ and a further assumption that $w_i = f(E_i)$ and $\frac{\partial w}{\partial E} > 0$ $\frac{\partial^2 w}{\partial E^2} < 0$ and

$\frac{\partial D'}{\partial E} > 0$. Also define: $W = \sum_{i=0}^{\infty} f(E_i)$

Then we have $\frac{\partial U_i}{\partial G} = -\frac{f(E_i)}{\sum_{i=0}^{\infty} f(E_i)} + q'(G)$. This will then imply

$$\frac{\partial MU_G}{\partial E_i} = -\frac{f'(E_i) \sum_{i=0}^{\infty} f(E_i) - f'(E_i) f(E_i)}{\left[\sum_{i=0}^{\infty} f(E_i) \right]^2} = -f'(E_i) \left[\frac{\sum_{i=0}^{\infty} f(E_i) - f(E_i)}{\left[\sum_{i=0}^{\infty} f(E_i) \right]^2} \right] < 0$$

Appendix 2

In order to evaluate the OCR process, random sample analyses have been conducted. Since the data comes from different printed sources, a sample analysis has been conducted for all sources for relevant variables. Only data which have been part of the OCR process are included in this evaluation.

From the publication series *Kommunal Finanstatistik - Finland* for the years 1967-1972 a random sample was drawn in STATA (without replacement). In total 25 observations were sampled (0.8 percent of total dataset). I analyzed the variables population, state grants, tax rate and tax base. I detected no errors and all the sampled observations corresponded to the scanned material.⁵¹

Second, for the publication series *Årsbok för Sveriges kommuner* for the years 1967-1977 a random sample of 35 observations was drawn (0.6 % of total data set). I analyzed the variables population, state grants, tax rate and tax base. In total, this corresponds to 140 cell values. 3 cell values were spotted to include a false value. The error rate is 2.14 %.⁵²

Third, for the publication series *Statistisk Rapport - Finland* for the years 1973-1977 a random sample of 21 observations were drawn (0.9 % of total data set). I analyzed the variables population, state grants, tax rate, total public expenditures and tax base. One cell value contained an error (population); however, in total 3 cell values were infected since state grants and public expenditures are expressed in per capita and I use the population variable to transform these variables. This corresponds to an error rate of 2.86 % (in total 105 cell values). One “2” had been interpreted as a “7”. This is a result of bad must and faint printing especially for the number 2 in this publication series.

Fourth, for the publication series *Kommunala valen - Finland*, a random sample of 16 observations were drawn (1 % of total data set – but I excluded the observations from year 1977). I analyzed voter turnout and vote share for the right wing block⁵³. In total, one cell value did not correspond to the scanned statistics. I only analyzed the years, 1967, 1969 and 1973 since these are the years for which I have used OCR conversion. This corresponds to an error rate of 3.33 % (in total 30 cell values in the sample)

Fifth, for the publication series *Kommunala valen - Sweden*, a random

⁵¹ For the population variable, no sample analysis was performed if the observation belonged to a city since these variable values has been manually inputted from a different data source.

⁵² One cell value had taken the value 0 for state grants – but it should have a positive value. This is most likely due to the fact that I replace all missing values with 0 since an empty cell in the printed statistics corresponds to 0 state grants according to the definitions in *Årsbok för Sveriges kommuner* and the coding in my dofile. For the other two errors, one number in each cell were wrong.

⁵³ This was done by some manually calculation since the vote share for the right wing block is a variable I create by using information from other variables.

sample of 14 observations was drawn (1 % of total dataset – but observations belonging to any year after 1973 were excluded from the analysis). I analyzed voter turnout and vote share for the right wing block⁵⁴. No cell values contained a wrong cell value.

Finally, the variable Public expenditure was analyzed separately,⁵⁵ since it is defined with a two years lead in the Swedish data. For the public expenditure variable in *Årsbok för Sveriges kommuner* a random sample of 29 observations was drawn. Only the years 1967-1975 are included in the analysis since I have manually inputted the information for 1976 and 1977. Two cell values contained inaccurate information. This equals an error rate of 6.9 %. For the Finnish part, 23 observations were randomly drawn for the years 1967-1972 and no cell value contained inaccurate information.

In conclusion, there are some remaining measurement errors in the final data set that the reader should be aware of. The overall conclusion is however that the OCR process has worked rather satisfactory.

⁵⁴Again, this was done by some manually calculation since the vote share for the right wing block is a variable I create by using information from other variables.

⁵⁵This was done by mistake for the Finnish part.

Appendix 3

Main specification, reduced form estimates

Table 12: Reduced form - Tax rate

VARIABLES	(1) Taxrate	(2) Taxrate	(3) Taxrate	(4) Taxrate	(5) Taxrate
Constitutional change 1970	0.586*** (0.047)	0.984*** (0.242)	1.898*** (0.078)	0.252** (0.112)	0.258** (0.109)
Municipal merge during the year		-0.560*** (0.145)			-0.091 (0.059)
New municipality during the year		-0.619* (0.327)			0.291* (0.149)
Observations	10,907	10,907	10,907	10,907	10,907
R-squared	0.014	0.199	0.279	0.789	0.791
Clustered standard errors?	No	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	No	Yes
Municipal fixed effects?	No	No	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes
Number of Municipalities			1,446	1,446	1,446

Normal or clustered standard errors respectively in parentheses

*** p<0.01 ** p<0.05 * p<0.1

Table 13: Reduced form - Public expenditures

VARIABLES	(1) PubExp	(2) PubExp	(3) PubExp	(4) PubExp	(5) PubExp
Constitutional change 1970	46,525*** (11,416)	7,155*** (2,188)	63,358*** (6,619)	32,476*** (7,486)	47,590*** (15,110)
Municipal merge during the year		-16,783*** (4,984)			-3,133 (5,012)
New municipality during the year		-20,427*** (6,524)			-20,276** (9,276)
Observations	9,591	9,591	9,591	9,591	9,591
R-squared	0.002	0.948	0.025	0.074	0.293
Clustered standard errors?	No	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	No	Yes
Municipal fixed effects?	No	No	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes
Number of Municipalities			1,394	1,394	1,394

Normal or clustered standard errors respectively in parentheses

*** p<0.01 ** p<0.05 * p<0.1

Robustness Analysis

Table 14: First and second stage and reduced form estimation, without outliers, Tax rate

VARIABLES	(1)	(2)	(3)	(4)
	OLS Taxrate	IV-First stage Turnout	IV-Second stage Taxrate	Reduced form Taxrate
Turnout	0.017* (0.010)		0.039** (0.016)	
Constitutional change 1970		6.563*** (0.436)		0.258** (0.110)
Observations	10,835	10,835	10,829	10,835
R-squared	0.790	0.606		0.791
Number of Municipalities	1,436	1,436	1,430	1,436
Clustered standard errors?	Yes	Yes	Yes	Yes
Additional covariates?	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses

*** p<0.01 ** p<0.05 * p<0.1

Table 15: First and second stage and reduced form estimation, without outliers, public expenditures

VARIABLES	(1)	(2)	(3)
	OLS PubExp	IV-Second stage PubExp	Reduced form PubExp
Turnout	1,162*** (205)	2,368*** (244)	
Constitutional change 1970			14,463*** (1,435)
Observations	7,066	7,016	7,066
R-squared	0.533		0.557
Number of Municipalities	1,252	1,202	1,252
Clustered standard errors?	Yes	Yes	Yes
Additional covariates?	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes

Clustered standard errors in parentheses

*** p<0.01 ** p<0.05 * p<0.1

Table 16: Public expenditures per capita, OLS

VARIABLES	(1) capitaExp	(2) capitaExp	(3) capitaExp	(4) capitaExp	(5) capitaExp
Turnout	-0.242*** (0.025)	-0.121** (0.048)	0.149* (0.086)	-0.109* (0.057)	0.149 (0.100)
Municipal merge during the year		2.127 (4.650)			3.623 (4.736)
New municipality during the year		-3.579*** (0.486)			-0.373 (0.969)
Observations	9,591	9,591	9,591	9,591	9,591
R-squared	0.010	0.033	0.001	0.015	0.038
Clustered standard errors?	No	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	No	Yes
Municipal fixed effects?	No	No	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes
Number of Municipalities			1,394	1,394	1,394

Normal or clustered standard errors respectively in parentheses

*** p<0.01 ** p<0.05 * p<0.1

Table 17: Public expenditures per capita, second stage IV

VARIABLES	(1) capitaExp	(2) capitaExp	(3) capitaExp	(4) capitaExp	(5) capitaExp
Turnout	-0.152*** (0.040)	0.035 (0.094)	0.266*** (0.074)	-0.222** (0.092)	0.283 (0.175)
Municipal merge during the year		1.441 (4.521)			3.601 (4.662)
New municipality during the year		-3.918*** (0.519)			-0.312 (1.033)
Observations	9,591	9,591	9,571	9,571	9,571
Clustered standard errors?	No	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	No	Yes
Municipal fixed effects?	No	No	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes
Number of Municipalities			1,374	1,374	1,374

Normal or clustered standard errors respectively in parentheses

*** p<0.01 ** p<0.05 * p<0.1

Table 18: Second stage IV-regression, random 80 % sample of dataset

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Taxrate	Taxrate	Taxrate	Taxrate	Taxrate	Taxrate	Taxrate
Turnout	0.039** (0.015)	0.038** (0.016)	0.041** (0.017)	0.038** (0.017)	0.037** (0.017)	0.039** (0.016)	0.036** (0.016)
Observations	8,678	8,692	8,684	8,680	8,696	8,688	8,690
R-squared	0.788	0.790	0.793	0.788	0.788	0.787	0.790
Number of Municipalities	1,392	1,407	1,401	1,395	1,406	1,404	1,404
Clustered standard errors?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses
 *** p<0.01 ** p<0.05 * p<0.1

Table 19: Second stage IV-regression, random 80 % sample of dataset

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	PubExp	PubExp	PubExp	PubExp	PubExp	PubExp	PubExp
Turnout	7,640*** (2,421)	7,362*** (2,464)	7,182*** (2,272)	7,855*** (2,505)	7,414*** (2,451)	7,414*** (2,454)	7,747*** (2,390)
Observations	7,530	7,489	7,520	7,529	7,562	7,542	7,539
R-squared	0.269	0.283	0.298	0.276	0.319	0.303	0.411
Number of Municipalities	1,223	1,218	1,227	1,232	1,243	1,240	1,223
Clustered standard errors?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses
 *** p<0.01 ** p<0.05 * p<0.1

Table 20: OLS and IV-second stage regression, mean tax rate over one mandate period

VARIABLES	(1)	(2)	(3)
	OLS meanTaxrate	IV-Second stage meanTaxrate	IV-Second stage meanTaxrate
Turnout	0.028*** (0.010)	0.081*** (0.015)	0.087*** (0.016)
Observations	10,907	10,901	10,901
R-squared	0.773		
Number of Municipalities	1,446	1,440	1,440
Clustered standard errors?	Yes	Yes	Yes
Additional covariates?	Yes	No	Yes
Municipal fixed effects?	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes

Clustered standard errors in parentheses
 *** p<0.01 ** p<0.05 * p<0.1

Table 21: OLS and IV second stage regression, all variables mean over one mandate period

VARIABLES	(1)	(2)	(3)	(4)
	OLS meanTaxrate	OLS meanTaxrate	IV-Second stage meanTaxrate	IV-Second stage meanTaxrate
Turnout	0.009 (0.009)	0.009 (0.009)	0.015 (0.017)	0.020 (0.017)
Observations	3,708	3,708	3,252	3,252
R-squared	0.851	0.852		
Number of Municipalities	1,440	1,440	984	984
Clustered standard errors?	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses

*** p<0.01 ** p<0.05 * p<0.1

Table 22: OLS and IV-second stage regression, mean public expenditures over one mandate period

VARIABLES	(1)	(2)	(3)
	OLS mean Pub. exp.	IV-second stage mean Pub. exp.	IV-second stage meanPub. exp.
Turnout	4,440** (1,687)	4,269*** (919)	6,619*** (2,047)
Observations	10,792	10,788	10,788
R-squared	0.359		
Number of Municipalities	1,394	1,390	1,390
Clustered standard errors?	Yes	Yes	Yes
Additional covariates?	Yes	No	Yes
Municipal fixed effects?	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes

Clustered standard errors in parentheses

*** p<0.01 ** p<0.05 * p<0.1

Table 23: OLS and IV second stage regression, all variables mean over one mandate period

VARIABLES	(1)	(2)	(3)	(4)
	OLS mean Pub. exp.	OLS mean Pub. exp.	IV-second stage mean Pub. exp.	IV-second stage mean Pub. exp.
Turnout	1,114 (795)	6,453** (2,760)	3,413*** (910)	8,744** (3,792)
Observations	3,654	3,654	3,244	3,244
R-squared	0.076	0.529		
Number of Municipalities	1,390	1,390	980	980
Clustered standard errors?	Yes	Yes	Yes	Yes
Additional covariates?	No	Yes	No	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses
*** p<0.01 ** p<0.05 * p<0.1

Table 24: First stage IV-regression, different subsamples for merged and non-merged municipalities (top) and more populated and less populated (bottom)

VARIABLES	(1)	(2)	VARIABLES	(1)	(2)
	Turnout	Turnout		Turnout	Turnout
Constitutional change 1970	8.040*** (0.599)	7.443*** (0.670)	Constitutional change 1970	6.226*** (0.456)	6.101*** (0.439)
Treatment Period	0.148 (0.520)	0.414 (0.723)	Treatment Period	1.341*** (0.363)	1.669*** (0.364)
Observations	2,599	2,599	Observations	8,308	8,308
R-squared	0.791	0.797	R-squared	0.440	0.452
Number of Municipalities	255	255	Number of Municipalities	1,191	1,191
Clustered standard errors?	Yes	Yes	Clustered standard errors?	Yes	Yes
Additional covariates?	No	Yes	Additional covariates?	No	Yes
Municipal fixed effects?	Yes	Yes	Municipal fixed effects?	Yes	Yes
Year fixed effects?	Yes	Yes	Year fixed effects?	Yes	Yes
Clustered standard errors in parentheses *** p<0.01 ** p<0.05 * p<0.1			Clustered standard errors in parentheses *** p<0.01 ** p<0.05 * p<0.1		
VARIABLES	(1)	(2)	VARIABLES	(1)	(2)
	Turnout	Turnout		Turnout	Turnout
Constitutional change 1970	5.894*** (0.475)	5.505*** (0.435)	Constitutional change 1970	9.111*** (0.392)	8.661*** (0.455)
Treatment Period	1.574*** (0.362)	2.381*** (0.383)	Treatment Period	-2.017*** (0.335)	-2.703*** (0.287)
Observations	8,238	8,238	Observations	2,669	2,669
R-squared	0.469	0.484	R-squared	0.764	0.771
Number of Municipalities	1,227	1,227	Number of Municipalities	359	359
Clustered standard errors?	Yes	Yes	Clustered standard errors?	Yes	Yes
Additional covariates?	No	Yes	Additional covariates?	No	Yes
Municipal fixed effects?	Yes	Yes	Municipal fixed effects?	Yes	Yes
Year fixed effects?	Yes	Yes	Year fixed effects?	Yes	Yes
Clustered standard errors in parentheses *** p<0.01 ** p<0.05 * p<0.1			Clustered standard errors in parentheses *** p<0.01 ** p<0.05 * p<0.1		

Appendix 4

To evaluate the accuracy of the data for all years and for all variables, some tables with more detailed descriptive statistics are presented in this section.

Table 25: Mean, for each year

	(1)	(2)		(1)	(2)
	Finland	Sweden		Finland	Sweden
Turnout			Municipal tax rate		
1967	80.17	83.51	1967	12.84	11.10
1968	80.13	83.47	1968	13.26	11.35
1969	78.94	83.46	1969	13.82	11.74
1970	78.91	83.44	1970	14.13	12.24
1971	78.90	88.74	1971	14.58	13.06
1972	78.94	88.73	1972	14.83	13.69
1973	78.60	88.73	1973	15.05	13.77
1974	78.55	91.00	1974	15.35	14.10
1975	78.66	91.00	1975	15.61	14.57
1976	78.67	91.00	1976	15.98	14.89
1977	80.80	90.97	1977	16.10	15.11
Observations	5152	5755	Observations	5152	5755

Table 26: Mean, for each year

	(1)	(2)		(1)	(2)
	Finland	Sweden		Finland	Sweden
Taxbase in thousands			Public expenditures		
1967	331117.6	81418.9	1967	93631.8	25748.6
1968	268068.0	92337.7	1968	79722.5	34379.1
1969	390502.4	97959.1	1969	112301.6	67624.8
1970	429228.2	110987.3	1970	125693.6	71515.9
1971	448070.9	193288.1	1971	134509.0	74188.8
1972	471444.6	213912.9	1972	147310.9	155333.4
1973	590925.3	227118.9	1973	133961.1	171483.2
1974	618743.7	387904.3	1974	143207.4	163066.9
1975	662480.0	414474.1	1975	185213.9	186248.9
1976	702489.9	394596.6	1976	178673.4	185744.8
1977	701844.4	425206.1	1977	215899.6	201178.5
Observations	5152	5755	Observations	5143	4448

Table 27: Mean, for each year

	(1)	(2)		(1)	(2)
	Finland	Sweden		Finland	Sweden
Number of inhabitants			State grants in thousands		
1967	7931.3	7319.7	1967	17071.7	1154.7
1968	7933.4	7255.8	1968	14625.7	1266.0
1969	9332.5	7592.1	1969	17725.1	1437.2
1970	9435.8	7921.0	1970	19129.3	1477.9
1971	9459.1	14688.2	1971	20728.1	2894.3
1972	9394.0	14503.3	1972	23576.7	3092.2
1973	10176.8	15123.9	1973	29878.4	3397.5
1974	10295.9	24759.7	1974	36481.8	6338.0
1975	10205.6	24796.0	1975	45164.8	6393.7
1976	10304.1	24505.8	1976	48096.9	5463.4
1977	10465.4	24763.0	1977	49202.0	5679.6
Observations	5152	5755	Observations	5152	5755

Table 28: Mean, for each year

	(1)	(2)		(1)	(2)
	Finland	Sweden		Finland	Sweden
Vote share right wing-block			Vote share left wing-block		
1967	57.93	51.51	1967	40.93	43.09
1968	58.07	51.54	1968	40.79	43.03
1969	61.65	51.05	1969	34.22	43.34
1970	61.64	50.94	1970	34.59	43.44
1971	61.55	50.32	1971	34.47	46.09
1972	61.66	50.34	1972	34.35	46.08
1973	62.76	50.43	1973	36.11	45.97
1974	61.07	50.90	1974	36.03	47.28
1975	62.92	50.90	1975	36.20	47.29
1976	61.01	50.75	1976	36.05	47.44
1977	58.64	51.16	1977	35.97	47.05
Observations	5134	5755	Observations	5134	5755

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